

PHILOSOPHICAL EMPIRICISM:

CONTAINING

REMARKS ON a CHARGE of PLAGIARISM
respecting Dr. H—s,

INTERSPERSED

With various Observations relating to

DIFFERENT KINDS OF AIR.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

Vivitur ex rapto. Non hospes ab hospite tutus.

OVID.

LONDON:

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PHILOSOPHICAL ENQUIRY

CONSTITUTION

REMARKS ON THE

REMARKS

ON THE

WITH A

DIRECTORY

BY JOSEPH PRITCHARD

Author of the

1791

LONDON

Printed by J. Johnson, Strand

and J. B. Nichols, St. Pauls Church-yard

A D V E R T I S E M E N T .

THOUGH this piece was originally intended to answer a temporary purpose, it is likewise calculated to refute some prevailing mistakes concerning the doctrine of *air*, and therefore will, I hope, be of use in establishing fundamental and just principles in this branch of Natural Philosophy, which is now become an object of very general attention.

I have not published the name of my antagonist at length, partly because I am really ashamed of such a contest; and also because I would not do him any more injury than I was obliged to do in my own justification. It will not be expected, I hope, that I should be quite grave and serious through the whole of this affair. I have been, I think, sufficiently so at the beginning; but the occasion did not require it throughout: and, indeed, it was not in my power to treat this very absurd and ridiculous accusation, but with a great mixture of ridicule and contempt.

Since the writing of this pamphlet, Mr. Godfrey (of whom I made some inquiry concerning the conversion of earth into water, men-

ADVERTISEMENT.

tioned p. 57.) has been so obliging as to favour me with part of a quantity of earth that had been produced from distilled water, weight for weight, by that celebrated chemist his grandfather, the cotemporary of Mr. Boyle, and his fellow-labourer.

This earth, I find, yields *fixed air* in great plenty, by the heat of a burning lens in quicksilver, as well as by means of the acids. And when it is made into a paste with spirit of nitre, it yields more air, the greatest part of which is also fixed air. This experiment I barely announce at present, as exhibiting a new fact respecting the generation of *fixed air*, that cannot be published too soon.

THE

T H E
I N T R O D U C T I O N,

C O N T A I N I N G

The **LETTERS** that passed between the **AUTHOR,**
Dr. H—s, and **Dr. BROCKLESBY.**

IT is with much reluctance, as several of my friends can witness, that, after withstanding, as long as I could, their earnest remonstrances on the subject, I have been induced to make this appeal to the public; I having been willing to think it unnecessary, and they insisting upon it that it was absolutely necessary. At length I yielded to their reasons. The case is as follows.

Before I left London, in the spring of the present year, in which my acquaintance with Dr. H—s commenced and terminated, I was told it was reported, that some of my new experiments, of which I had sent an account to

the Royal Society, subsequent to my acquaintance with him, were only the result of his *general principles* concerning air; and Dr. Brocklesby, when he saw some of my new experiments, in the company mentioned in my letter to him, said of them *all*, without distinction, that they were those that Dr. H——s had shewn. But as I knew that Dr. H——s and myself held no common principles concerning air, as Dr. Brocklesby had not the character of being the most accurate man in the world, and I thought that my character for *veracity*, at least, was sufficiently established, I intirely neglected the insinuation, and really thought no more about the matter, till I was informed, by a letter from London, while I was in the country, that the report of my having taken several things from Dr. H——s gained ground.

Knowing, however, that there could be no foundation for this charge, I continued to pay no attention to it; and though, upon coming to town, I found it was in every body's mouth, and my friends urged me to make some inquiry concerning it, I neglected to do it for a considerable time; thinking that the publication of my *second volume of Observations concerning Air*, which was then nearly printed off, would speak for itself, and satisfy every body who

who should peruse it, that the narrative carried its own evidence along with it.

But I was told that the charge of plagiarism, absurd as it was, had been so long, and so industriously circulated, without having been contradicted by any proper authority, that it had really gained much credit; that many persons, without distinguishing times or dates, had publicly, and with great confidence advanced, that even *all* my discoveries had been taken from the same Dr. H——s. On this account, not only my friends, but persons with whom I had no strict connection, assured me that, in their opinion, it really behoved me to make some regular inquiry into the business. Accordingly I did, at length, though with great reluctance (still hoping that there could be no necessity for any appeal to the public upon the subject) set myself about it; when I presently found what the following letters will specify.

TO DR. BROCKLESBY.

Dear Sir,

The business I write to you about is so irksome to me that I have deferred it as long as possible, hoping there might be no occasion to

give you any trouble on the subject. At length, however, I have been persuaded by my friends to do it.

It is reported, I find, that some experiments, which I have lately exhibited as my own, I took from Dr. H——s, and wherever I inquire about it, I am told that you charged me with it when you saw my experiments at Shelburne-house, in company with Dr. Fothergill, the two Dr. Watsons and Doctor and Mr. John Hunter*.

Now, as you did not at that time charge me with any plagiarism, but only supposed that both Dr. H——s and myself had made the same discovery, and did not even say that you had yourself seen those experiments of Dr. H——s's, I must beg the favour of you to tell me what those *common experiments* were, and by what authority you took upon yourself to say, that the experiments you then saw were the same with those of Dr. H——s's, which you had not seen: for if I remember right, I shewed you *several* at that time, which were not mentioned in my first volume.

I have not heard that Dr. H——s himself charges me with having taken any thing from him;

This was on the 23d of May, 1775.

him; and with respect to the principal thing which I then shewed you as new, it is impossible that he should have claimed it; when, as I immediately told you, in the presence of the gentlemen abovementioned, it was but a little time before, that he had hesitated to admit the facts when I mentioned them to him; as, indeed, I should have done myself a little before that, had any other person mentioned them to me; the discovery of them having been perfectly accidental, and affording no foundation for merit whatever.

What he advances in his printed *Syllabus* is the very reverse of my ideas on that subject, and, in my opinion, is contradicted by the experiments I then shewed you. Indeed, it is now abundantly evident, that Dr. H——s and myself have hardly one common idea concerning air; so that if he be right, most of my discoveries are, what he has thought proper to call them, mere *conceits*; and if I be right, his general doctrine is entirely chimerical and false. On this account, it is hardly possible that we should have taken any thing from each other; except that he has adopted some things contained in my *first volume*, the second edition of which had been published some time before I had so much as heard the name of Dr. H——s.

In this business, therefore, there must have been some mistake (I hope not yours) which I am told it behoves me to inquire into. I am, with real regard,

Dear Sir,

Your very humble Servant,

Shelburne-house,

Nov. 30, 1775.

J. PRIESTLEY,

This letter I delivered to the Doctor at the Royal Society, on the day in which it is dated; and the same day, having received farther information concerning the business, I wrote the following letter to Dr. H——s.

Sir,

I have this day been informed, from undoubted authority, that you have charged me with having published, as my own, experiments, what I learned of you; but though I have inquired of several persons, who all agree in the fact, of the charge in general, none of them can tell me what the particulars of it are. I must, therefore, beg that you would yourself inform me concerning them. A man of honour would have given me an opportunity

tunity of vindicating myself, before he had published my accusation to others.

I am, Sir,

Your very humble servant,

Shelburne-house,

Nov. 30, 1775.

J. PRIESTLEY.

The next day I received the following answer from Dr. Brocklesby, and on Dec. the 3d, that which follows from Dr. H——s.

Dear Sir,

The experiments which I saw you institute at Shelburne-house appeared, so nearly the same with a greater variety of such as I had seen in three courses of chemistry given by Dr. H——s, that, in justice to my absent friend, I was urged, possibly, to violate the laws of hospitality, by declaring in the instant, that none of the divers experiments you was then pleased to exhibit were novel to me, except one concerning the Swedish fluor.

Whether your discoveries were prior to those of Dr. H——s I must leave to the determination of others, it being, at this distance of time, not easy for me to ascertain to whom the priority of these claims belongs.

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Whenever this subject has occurred in conversation, I have repeated what I had, with the most pure intentions, declared in your presence; never apprehending you had cause of offence, on subjects wherein, by your own declaration, you and Dr. H——s entertained notions totally repugnant.

I sincerely wish your philosophical improvements may obtain every merited honour: at the same time I should feel myself unjust to suppress candid applause to another gentleman, of whose unwearied labours I have been a constant witness more than a year and half past. I am, with great respect,

Dear Sir,

Your most obedient humble servant

Norfolk-Street,
30 Nov. 1775.

RICHARD BROCKLESBY.

Sir,

Nine months are elapsed since I informed you, in plain, but the least offensive terms, that I wished to decline your visits and correspondence. You know the motives of a conduct so candid, and with all so repugnant
to

to my own interest as a teacher. You now cannot seriously expect that I should repeat what you well know—that I should enumerate the things which you assume as your own, and which I had previously shewn and taught.

If any other gentleman had proposed the question contained in your letter, an answer would be necessary; and I should commence it with comparisons of the dates of Dr. Priestly's rapid publications, with the dates of my courses of chemistry.

For the future I will add to the charge against you, that you have treated others as you have treated me; and that your originality in experiments consists chiefly in the knack of rendering the phenomena, which all practical chemists have observed and understood, perfectly mysterious and surprizing to others.

The only part of your letter, then, which requires an answer, is that wherein you hint that a man of honour would remonstrate to you, instead of uttering the truth to others. Herein your notions of honour and mine differ widely. I speak freely such truths as can be well vouched, but I never remonstrate, except

cept when a gentleman has inadvertently offended.

I am, Sir,

Your humble Servant

Greek-street, Soho,

Dec. 3, 1775. BRY. H—s.

This letter (the gross rudeness, manifest shuffling, and absurdity of which, will hardly impress my reader in his favour, and may, perhaps, make some of his friends blush for him) giving me no sort of satisfaction with respect to the particulars of the charge of which I was in quest, I thought it necessary to interrogate Dr. Brocklesby more distinctly; especially as he owned that he had of *himself only*, and not, as I had imagined, through the medium of some third person, asserted the identity of my experiments with those of Dr. H—s. I therefore sent him the following letter, which brought an answer not more satisfactory than the former, except that I was convinced by it, that nothing more satisfactory could be procured on the subject.

Dear Sir,

I am glad to find by your letter, that I am to look no farther than to *yourself* for the evidence

dence of the experiments I shewed you at Shelburne-house having been the same with those Dr. H——s had exhibited before that time. And as you have not yet answered the question which I took the liberty to propose to you (since experiments that *appear nearly the same* with others, may, in reality, be essentially different from them) and as Dr. H——s himself has refused to give me any satisfaction on the subject, I am obliged to repeat my request. But to make the trouble of satisfying me more easy to you, I shall be a little more particular in this letter than I was in my last.

The first experiments that I had the pleasure of shewing you were those by which I shew in what manner to apply the test of nitrous air, to ascertain the purity of atmospheric air, which is described in my first volume, and the manner of firing inflammable air with, or without common air, which I learned of Mr. Cavendish. None of those, therefore, are to the present purpose. After this, the only thing I exhibited, which I declared to have discovered posterior to the publication of my first volume (exclusive of the experiments on the *fluor acid*, which you acknowledge you had not seen with Dr. H——s) were

were experiments relating to quite another kind of air.

Now as, by your own account, you were able to pronounce immediately upon the identity of those experiments with those you had seen of Dr. H——s's, and have repeated the same thing whenever the subject has occurred in conversation since, you must be able to tell me now what those experiments were. Please, therefore, to answer the following questions.

1. From what materials did I tell you that I procured that air?

2. What name did I give to it?

3. What were the peculiar properties of it?

4. In what manner did I demonstrate those properties?

I propose these questions so distinctly, because unless you can answer them with precision now, it cannot be thought that you were able to pronounce on the subject with sufficient precision before. Please also to tell me, as nearly as you can recollect, how long it was before you saw the experiments above-mentioned

mentioned with me, that you had seen the same with Dr. H——s. Was it in his first, in his second, or in his third course? for you mention three of them.

I will add, that the experiments which I exhibited to you were so very remarkable, and so exceedingly different from any others, of which any printed account was then published, that they must have struck you in a particular manner; and therefore you cannot but remember pretty nearly when it was that you first saw them. I do not even think it possible that they could have been exhibited at any public lecture in London, without occasioning so much conversation among philosophers upon the subject, that I must myself have heard of them.

You need not make any apology for what you call *violating the laws of hospitality*, provided you have scrupulously observed (as I am fully satisfied you have done *intentionally*) the much more important laws of *truth*.

I am,

Dear Sir,

Your obedient humble Servant,

Shelburne-house,

J. PRIESTLEY.

Dec. 4, 1775.

THE ANSWER.

Sir,

Your letter found me yesterday sent for to attend a lady ill of a fever at Guildford, and I did not return till this day noon, and in the first moment of leisure, at 10 o'clock at night, I now sit down to answer your letter, and I hope it may close our correspondence on this subject of controversy, from which my temper is truly averse.

I must, in the first place, submit to your reconsideration the following paragraph in your letter. "I propose these questions so distinctly, because, unless you can answer them with precision now, it cannot be thought, that you were able to pronounce on the subject with sufficient precision before."

Hereupon, I take leave to observe, that this conclusion is not admissible, and that an opinion given in the instant, and in your presence, whilst the facts were before me, may have been altogether just, although, at the distance of many months (having seen, both before and afterwards, frequent and various combinations of similar experiments) I do not perfectly recollect every experiment then made, nor even all the new names you might have given

given to appearances, which were familiar to me. I will not, therefore, attempt to answer the following questions, which might involve me in subtilties, or at least lead you to further perplexities, rather than clear up the subject.

1. From what materials did I tell you, that I procured that air?

2. What name did I give it?

3. What were the peculiar properties of it?

4. In what manner did I demonstrate those properties?

But your 5th and last question I will most readily answer. Dr. H——s, in the first course of Chemistry, June 1774, read his first lectures on the subjects of air, fixed air, inflammable air, the elastic matter of acids, of alkalies, of phosphorus, ethers, and on phlogiston, light, and fire; on all which subjects he entertained (to the best of my recollection) the opinions he now advances, and he supported these opinions by various striking experiments, and by some of those you shewed, among others.—And having seen the manuscripts from which he read in his first course, I had no more to say.

and which were voluminous, I am persuaded that very many of his conclusive experiments were made at a period anterior to his first course. Thus far I speak what is known to others of his pupils; but of my own knowledge, I can affirm that, in private conversation, he has repeatedly discussed, and debated these subjects with me, so as to have converted me from my formerly-imbibed opinions of fixed air, inflammable air, and phlogiston, for several years previously; and I remember particularly his conversations concerning Mr. Woulfe's method of saving the acid, ethereal, and alkaline elastic fluids, published years ago; in divers of which conversations he attempted to convince me of the nature of these fluids, always expressing the highest veneration of his favorite philosopher Mr. Cavendish, whose genuine taste and precision in conducting experiments, and his philosophical inductions, he was often wont to say were truly worthy of a disciple of Bacon, or the immortal Newton; and that modern Philosophy, in his opinion, owed more to Mr. Cavendish, than to any other man now living, except Dr. Franklin. In consequence of a variety of thoughts, suggested to him, by a careful perusal of Mr. Cavendish's works, Dr. H——s, in his first, as well as in his succeeding courses, brought experiments conclusive with me, so that I
feel

feel myself as much convinced, as the nature of these recondite matters admits of, that his notions of elastic fluids, distinct from air, are founded in nature, and that acid, whether vitriolic, muriatic, or vegetable, is an elastic fluid when detached, and that, even however combined with phlogiston, all these, together with the microcosmic acid, may form a combustible vapor, incoercible in the ordinary processes of chemistry, but which may be detained in proper vessels to serve for experiments.

And I apprehended that such combustible vapour (whether in making ether, or metallic solutions, or by decomposing sulphur with iron filings moistened with water, or if even Knuckel's phosphorus, formed or detached by various other artifices, devised by Dr. H——s, to confine phlogistified vapour) will burn in open vessels, in that part immediately in contact with the atmosphere; and I learned that these elastic vapours when mixed in various proportions with common air, and approached by flame, shall displode, and cause a loud noise in going off, and leave the air newly combined with some principle that was in the vessel, so that it soon shall become fixable air; in almost all which he candidly repeated his obligations to Mr. Cavendish.

The acid of nitre he all along considered nearly in the same manner as the other acids, with this difference only, that with the elementary acid of nitre he ever impressed the notion, that somewhat which operates like air in all combustions, and on phlogistic bodies, or possibly that air itself is combined. This too he has shewn by experiments with nitrous acid and spirits, oils, phosphorus, metals, &c. with all which visible fire is produced by his curious processes. And with a number of other bodies only heat, not fire, was produced.

He also frequently remarked the phenomena of mixing air with the nitrous vapor, which he did in a very simple manner, by only unstopping the bottle of his strongest nitrous acid in a quiescent air, or remarking the like appearances in a process for pfirmieson, and several others.

He demonstrated that sal-ammoniac is made by combining volatile alkali with muriatic acid, and that this combination takes place in the great elaboratory of nature, in the volcanos of Etna, and wherever else that salt is found in nature, as well as the processes of art for making this great article of commerce. And that in every possible combination of acid vapor

por with volatile alkali in vapor some neutral salt is produced. This he also explained by forming at his lectures the elastic vapor of strong acid and alkali.

Now having fairly given you this minute detail of such experiments made in Dr. H——s's course of lectures, ask yourself if those you was pleased to exhibit at Shelburne-house could appear altogether novel to me: for I apprehend your giving other names to such experiments, or using a smaller or neater apparatus, did not constitute any important new discovery.

I will now end this very irksome business, with one remark, that the most sublime philosopher, who weighed distant worlds as in a balance, and taught wondering mortals many of the most secret laws of nature, as they operate on all matter, had so great an aversion to dealing in controversy, that I know, on good authority, the world had like to have been deprived of the *Principia*, when he apprehended the publication of that book might involve him in any altercation with his contemporaries; whilst, in our days, on the contrary, I am, against my will, drawn into this long and tedious letter, to settle whether a philosopher, high in modern rank, has the

exclusive privilege in this or that phlogisticated vapor of the mineral, vegetable, or animal kingdoms. I know this is my first literary dispute, and that it shall also be my last, for I will say no more, but that I am,

Dr. PRIESTLEY'S

Humble Servant,

Norfolk-street, 5th

Dec. or rather 6th,

at 2 o'clock morn.

RICH. BROCKLESBY.

From this letter it is but too apparent, that Dr. Brocklesby had not been able to distinguish what he saw with me from what he had seen with Dr. H——s, and therefore that no sort of stress can be laid on his testimony. Had I urged him any farther, and (like Daniel with respect to Nebuchadnezzar) told him what he himself had quite forgotten, or rather had never rightly apprehended, viz. that he had seen with me with a species of air which I had procured from earth and spirit of nitre, and which I had called *dephlogisticated air*; being about five times as pure as common air; that a mouse had lived in a quantity of this kind of air five times as long as it could have done

done in an equal quantity of common air; that a candle burned in it with five times as great splendor as in common air; and that when a quantity of inflammable air was fired in it; the report was even fifty times louder than it was in common air; still, situated as he was, and so little able (as his letter demonstrates) to distinguish what he saw, he might have persisted in what he had incautiously once asserted, and therefore, without the least violation of integrity, might have affirmed that he had seen all those things with Dr. H—s; though according to his own Syllabus, there could not, in nature, be any such thing. But I was far from wishing to push the Doctor upon this precipice. All I had occasion for was barely to set aside his testimony against me, for which his present utter ignorance of what he saw with me (though things of so very remarkable and striking a nature) is abundantly sufficient.

I must not close this article without congratulating Mr. Cavendish on his acquisition of the profound admiration of so competent a judge of philosophical merit as Dr. H—s. But though he knows that I believe him to be very deserving of the encomiums that Dr. H—s, and Dr. Brocklesby have paid him, I rather think that his feelings upon the occasion

sion will not be very different from those of Dr. Franklin, in a situation that will be mentioned hereafter, and that it would have given him more pleasure *laudari a laudato viro*.

Finding myself, after all the pains I had taken, to lie under an accusation of so very vague and undescribed a nature; having endeavoured in vain to procure a *copy of my indictment*, either from my accuser, or the witness; and not knowing how far this unknown charge may extend, I must endeavour to make it out myself, in the best manner that I can, from such materials as the recollection of the whole of my intercourse with Dr. H——s can supply me with; for which purpose I must go over it all, and especially our conversations on philosophical subjects. This plan will oblige me to mention several things which must appear to his disadvantage, and which I should not otherwise have mentioned. But my situation is such, as does not allow me to have recourse to any other method, more favourable to him. Had his accusation been *distinct*, and confined to any certain number of articles, I should have answered to those articles only.

SECTION

SECTION II.

*A general account of my intercourse with Dr.
H——s.*

It was some time in January, of the present year 1775, that, being at the Royal Society, I first heard the name of Dr. H——s from Dr. Brocklesby, who told me that he was a person highly deserving my notice, as an excellent chemist, and especially as one who had made several discoveries concerning air. I asked him what particular discoveries, of value, he had made. He replied that he had discovered fixed air to consist of common air and phlogiston. I answered, that that was very far from being my idea of the matter, and freely intimated to him, that a person who maintained an opinion so contrary to all probability could not be much of a philosopher, or have given much attention to the subject. Still, however, the Doctor pressed me to be introduced to him, and, with much reluctance, as he can witness, I did, at length, consent to dine with him on the day that Dr. H——s was to open his next course of lectures, which was the 6th of February following, that we might go together.

In the mean time having inquired of a very respectable friend whether he knew any thing of this Dr. H——s, whom Dr. Brocklesby had recommended to me, he advised me to have nothing to do with him. Upon this I gave over all thoughts of attending his lecture, as some of my friends well know. However, my evil destiny, aided a little by *curiosity*, and such a desire of knowledge, as misled our first parents, helped me, at length, to get rid of my scruples; concluding that, though Dr. H——s certainly knew very little about *air*, he might be what is called a good *chemist*; and with such a person I had long wished to form some acquaintance, being conscious of my own deficiency in that kind of knowledge.

Accordingly, after dining with Dr. Brocklesby, on the day mentioned above. I was introduced by him to this extraordinary man, who received me with marks of the greatest deference and respect, and put me not a little to the blush by introducing his compliments to me in the course of his lecture, as well as into his conversation.

Upon telling Dr. Franklin, the next morning, where I had been the evening before, he told me that he had once attended one of those introductory lectures of Dr. H——s (four of which

which he gave *gratis* in this course) and said, "Pray, did he not pay you some compliments in the course of his lecture?" I said yes. He replied, "I thought he would, for he paid me so many, that I was quite ashamed, and really had a more unpleasant feeling, than I had during all the time of Wedderburne's lying abuse of me, before the Privy-Council. I believe, however," added he, "that the man may be a good chemist, and his acquaintance may perhaps answer your purpose." Seriously, as this great man is now engaged, he will smile when he sees an account of this incident in print, as well as at the ridiculous contest into which I have been drawn.

In the manner in which Dr. H——s delivered this lecture there was an appearance of *modesty* and *diffidence*, with which I was much pleased; and, looking upon him as an industrious and ingenious man, wholly devoted to his profession, who had expended vast sums of money on his apparatus and experiments; and seeming, by his looks, to have wasted his constitution, as well as his fortune in these pursuits, I really had a strong feeling of compassion for him, and made a point of recommending him to my acquaintance, as a *modest and sensible lecturer*; and this I did pretty warmly

warmly (as I am known to be apt to do, whenever I conceive a liking for any person) and this several of the nobility, other persons of large fortune and distinction, and my philosophical and literary acquaintance in general, can witness. Nor have I, to this day, taken the least pains to unsay any thing that I then said in his favour, or have said any thing else to his disparagement; except when I have been particularly urged to it, by something occurring in conversation, that made it necessary for me to do it, in order to my own vindication. Indeed, I was ashamed to retract what I had, in my incautious zeal, so warmly advanced.

In our conversation after the lecture, Dr. H——s, in the presence of Dr. Brocklesby, expressed, in the strongest manner, the sense he had of the honour that I did him by my attendance on his lecture, and in a very handsome manner made me a tender of his best services, in case he could be of any use to me. I told him that, not being a practical chemist, having never had a proper laboratory, or seen much of the usual processes, I wished to have an opportunity of observing some of them: but that I more especially was in want of *chemical articles*, such as I could not easily procure at the shops, or on the preparation of which

which I could not absolutely depend; and therefore should think myself very much obliged to him, if he would supply me with such things as I might occasionally want in the prosecution of my experiments, and that I should very thankfully give him whatever price he chose to ask.

This he readily promised to do, and added, that if I would do him the pleasure to call upon him, I should be sure always to find him at home before dinner, and that there would never fail to be some process or other in his laboratory, which I might examine at my leisure. In return for this obliging offer, I desired that he would give me the pleasure of his company at Shelburne-house, where I would endeavour, in return, to entertain him with such experiments as I made. But this, alleging he had no time to spare, he civilly declined.

From this time I called upon him occasionally, took of him such articles as I wanted, always gave him his price (concerning which I was intirely ignorant) and always expressed myself much obliged to him. I seldom stayed with him more than a quarter of an hour at a time, sometimes not more than a few minutes; being in haste to make my experiment with

with the substance that I procured of him : and I do not think that all the time I ever spent with him exceeded four or five hours. Indeed I very seldom staid any longer than while he was either finishing what I found him about, or while he was employed in weighing, making up, and labelling the several articles I took of him. Exclusive of this, I do not think that I spent more than a single hour with him in all; my own time being as fully employed as his. And the time I spent with him in this manner was chiefly out of regard to civility and propriety; thinking it would not be decent to make the same use of his laboratory, as of a common shop; always running away the moment that I had got what I wanted.

The second, which was the last time of my attendance on his lecture, I put myself to some inconvenience to do it, and really did it from no other motive, but that I thought I should oblige him by my countenance; and though I had not the vanity to think that I was doing him all the *honour*, and all the *pleasure*, that he told me my visits would do him, I was willing to give him the gratification that he seemed to promise himself from them.

At one time I was induced to make a longer stay with him than usual, by the coming in of
Mr.

Mr. Delaval, whom I had not had the pleasure of seeing before, and whom I was much pleased with having this opportunity of seeing. And I mention it to give my readers some idea of the manner in which he, at that time, usually treated me, that they may compare it with the stile of his letter to me.

Upon mentioning my name to Mr. Delaval, which he did in a manner that seemed to shew he had some kind of satisfaction in doing it, he said, "You see, sir, all men of note find me out at last," or words to that effect. Also, when, in the course of one of our conversations, I had occasion to ask him whether he happened to have a copy of my book at hand, he replied, with that formality, of which all who are acquainted with him know that he is capable, "Do you think I could possibly be without so very capital a performance upon the subject"?

This compliment was, to be sure, awkward enough; but I did not take it to be meant *ironically*, as there was nothing else in the conversation that could bear such a construction. How he can now reconcile these encomiums with his calling the principal discoveries contained in the same book mere *conceits*, and with his saying that what I have done consists chiefly in

in the knack of rendering the phenomena which all practical chemists (and himself, no doubt, who is at the head of them all) have observed and understood, perfectly mysterious and surprizing to others (that is, those who are not practical chemists) I leave to him, as a practical chemist, to make out. After these compliments, was it possible for me to imagine that my company could be so very tiresome to him, as he has since affirmed?

I had not called upon Dr. H——s more than two or three times, before I began to perceive that his appearance of *modesty*, and his extreme *deference* and *complaisance*, began to wear off; so that, like the fox with respect to the lion, in the fable (if he will like the comparison) he began to be much more at his ease, and his natural character and turn of mind became sufficiently conspicuous. For, from an extreme of deference and respect, he advanced, by degrees, to such a pitch of *assurance*, and such airs of *conceit*, and *self-importance*, as I have seldom observed in any man; perpetually boasting of the discoveries he had made (but without mentioning any of them) complaining loudly of the great expence he had been at for the sake of promoting science, and of the low illiberal taste of the age, discovered by his not receiving

ceiving proper encouragement; speaking contemptuously of other persons of his profession, and with particular indignation of many persons (whose names, however, he never mentioned) who had stolen their discoveries from him, without having made any acknowledgment of it in their publications *.

Such topics, and such a turn of conversation, into which he was perpetually falling, gave me, I own, no very favourable idea of my new acquaintance. But still I made allowance for this conceit, and bore with it, as being, in some measure, incident to persons who give their whole attention to a single thing, in which they are allowed to excel, who have not seen much of the world, and who have, therefore, had no opportunity of acquiring that *liberal turn of mind*, which is the greatest ornament of true science.

* I always joined with Dr. H—— in condemning this kind of conduct, and assured him that whatever obligation I should be under to him, I should certainly acknowledge it; and my book will prove that I have done so in the amplest manner. My second volume, which is now printed off, was written at a time when I was very far from being satisfied with his conduct, though I had not heard of his claims upon me. But though he has used me very unhandsomely, I have some obligation to him for the materials he allowed me to purchase of him, and therefore I do not wish to retract what I have said.

I now

I now come to the catastrophe of our acquaintance, of which he has given very different accounts, and concerning which I have formed different conjectures, in consequence of viewing it in different lights, as I should do any remarkable appearance in philosophy. As I always told him, when I applied to him for any substance, or preparation, what I wanted it for; I sometimes asked him whether he could not recommend to me something else that was likely to answer my purpose better; and sometimes he would tell me, and sometimes he declined it; almost always concluding the conversations we had upon these subjects with telling me that *I must attend a complete course of chemistry*. I always replied, that I had not time for it; never suspecting what he was aiming at all the while; till, at length, upon his urging me on this head more strongly than before, and my telling him more peremptorily than before, that I really could not spare time for any such thing, he said very abruptly, that "his time was so much taken up with necessary business, that, without meaning any person in particular, he was obliged to come to a general resolution, to answer no questions but such as he was paid for." This, in a moment, disclosed to me (as I then concluded) what I was astonished I should not have discovered before, viz. that his

his little object had been to get my subscription for attending his course. Disconcerted as I was, I had the presence of mind to commend his resolution, as very necessary for a person of his many engagements; and after this I called upon him no more.

Till this last conversation, which was in his own house, while he was shaving himself, and consequently did not engross any of his valuable time, I had seen nothing in his behaviour (making the reasonable allowances above-mentioned) that could give me offence, nor did I perceive any mark of his having conceived the least dislike to my visits. Even this very last time that I was with him, part of his conversation was, to all appearance, very friendly. He then mentioned to me, particularly, *Mr. Wilson's book on Phosphori*, and expressed the strongest disapprobation of his treatment of me in it; saying he *bated such things* among philosophers; and added, that he had freely told a friend of Mr. Wilson, who would be sure to tell him again, that, besides the malice of the thing, he was quite wrong with respect to the fact.

Could I imagine that a man who talked to me in this manner was, at the same time, wishing to get rid of me? I therefore conclude, that

his determination was occasioned by the conversation that immediately followed this, and by which he found that I absolutely refused to attend his lecture; whether his view was merely to get my *subscription money*, which I then imagined to be his object, as those of my friends to whom I told the story can witness; or whether he meant to engage my attendance upon his lecture with a view to something farther, as I now conjecture, viz. that he might have the honour of being my instructor, and thereby have a pretence for laying claim to all my experiments.

That I *took up too much of his time*, I am satisfied is an after-invention; and in his letter to me he makes no complaint of that kind, but alludes to something else, which he says *I know*, but concerning which I can only form conjectures.

When I consider every thing relating to this business, I cannot easily satisfy myself with any hypothesis to account for Dr. H——s's behaviour to me. He is a man altogether unknown to the world. He has not distinguished himself by any philosophical discovery that I have yet heard of, and the airs he may give himself in his class, or in conversation, are nothing

nothing to the world at large. He may, in fact, be as great a man as Lord Bacon, Sir Isaac Newton, or Mr. Boyle; and if his performances should correspond to the idea that his printed *Syllabus* is calculated to give us of him, he must be a greater man than any of them, and indeed greater than all the three put together. But then this cannot be known to the world, till his experiments, proving the discoveries that he has announced, be actually made, and an account of them be published, which will require at least some months (though before that time his *subscribers* may have an opportunity of knowing whether he be, in fact, the great man that he gives himself out to be or not; and some of them, it can hardly be doubted, will have *zeal or indiscretion* enough to whisper the fame of their master, whatever injunction his modesty may lay them under) and during the time that I had the honour of his acquaintance, he had not so much as announced his importance to the world; for his famous *Syllabus* was not then published; so that even now, and much more so far back as the spring of the present year, he must be considered as *an obscure person*, to whom, consequently, the countenance of a person more known to the world might be supposed to be of use.

Now, with respect to myself, whether it has come to me by inheritance, or by acquisition, just or unjust, whether it is owing to good fortune, or desert, it is *fact*, that I have been some years in possession of the most respectable acquaintance that this country can furnish; and being naturally warm, and I will add constant in my attachments, it could not but be much in my power to befriend any man in the situation of Dr. H——s; who, one would imagine, would, therefore, rather wish to be brought forward by my friendship, than rashly make me his enemy. It is certainly the interest of every man who lives by a profession, and especially of one who, like Dr. H——s, has rivals in that profession, to have as many friends, and as few enemies as possible.

I therefore frankly acknowledge that I cannot clearly account for the fact, as a *phenomenon in human nature*; unless perhaps by adding to the conjectures above-mentioned, that he may have been practised upon by some of my enemies (for all men have enemies) or that, being possessed of an uncommon degree of conceit, and having but little knowledge of
the

the world and of mankind, to counteract the absurd effects of that preposterous passion, he has taken it into his head, that he should gain more by setting himself up as my *rival* in philosophical reputation, than he should do by availing himself of my friendship.

It appeared to me at the time, that he suspected me not to be quite sincere in what I had told him concerning my endeavours to serve him among my acquaintance, because they did not immediately wait upon him, or attend his lecture; which was a very unreasonable expectation. For a person who knows any thing of the world must have been apprized that, recommendations of this kind can only operate slowly, and that sufficient time must be allowed in all cases of this nature. For, at the same time that he told me that he was come to a resolution to answer no more questions but such as he was paid for, he said, "a greater mischief could not be done to a man, than to flatter him with false expectations of patronage and encouragement." After this I resolved not to do him any more mischief of this kind. But neither have I done him any mischief of a different kind; for I have never taken the least step to his prejudice. But, with respect to all these conjectures, I can only say with Logicians, *valeant quantum valere possunt*.

I shall conclude this section with acknowledging that this affair has contributed not a little to lower me in my own estimation, as I really imagined that my character was such, as could not but have been more respected by such a man as Dr. H——s, and that independent of my recommendation of him, he would even have thought my philosophical communications (of which all my acquaintance know me to be very liberal) a sufficient recompence for the little services that he could do me.

SECTION

SECTION III.

An account of what I saw or heard, of a philosophical nature with Dr. H—s.

I shall now proceed to recite the substance of all that I saw or heard, that bore any relation to philosophy, in the very short time that I spent with Dr. H—s; that the public may form some judgment of the probability of my having taken from him any thing that I have since published as my own. But really our conversation very seldom turned upon philosophy; most of the time that I was with him being taken up with complaints of the vast expence he had been at, and the little prospect that he had of getting his capital back again: tho' I must do him the justice to say, that he always spoke with the greatest contempt of *money*, calling it, to use his own words, mere *dirt* and *trash*, compared with *philosophy*. There only remains some little doubt, whether, in this, he had a view to his own money, or to mine.

Of his first lecture (which, of course, consisted of introductory matter, proper for be-

ginners) I remember nothing but his producing a variety of diagrams, in order to explain the nature of chemical attraction and repulsion, which he seemed to do with ingenuity enough.

In his second lecture, he did very little besides attempting to exhibit my experiments on alkaline air: but his apparatus being very ill contrived, he did not succeed to his wish. He was particularly embarrassed in consequence of using very long glass tubes, filled with quicksilver: but he told us that it was necessary to have them of that length, that when the mercury had subsided to its natural level, there might be a vacuum in the top of the tube, for the alkaline vapour to expand itself in. But in this, not only is his *reasoning* very absurd, but the *practice* is liable to lead the experimenter into a mistake, with respect to the real quantity of the air introduced into those long tubes. For my own part, I have seldom made use, for the same purpose, of tubes any longer than about nine inches, which are certainly both more commodious and more useful; and though the quicksilver compleatly fills these short tubes, it is necessarily displaced, and its room occupied by the ascending air or vapour.

But

But though he succeeded so ill in this experiment on *air*, I considered that the subject was new, and that it is only long practice that gives dexterity, and insures success in things of this nature. I cannot, however, forbear expressing my surprise on this occasion, that he should adopt my own method of exhibiting the alkaline principle, if it only tended to make "that mysterious and surprising, which, in the method that was known before to all practical chemists, was perfectly intelligible." In an address calculated for *students*, he certainly should have adopted a method the least mysterious possible.

The first philosophical conversation that I had with Dr. H——s was of his own introducing, in the presence of Dr. Brocklesby, on his favourite topic of the *constitution of fixed air*, on which we each of us gave our different opinions; he maintaining that it consists of common air and phlogiston, and I dissenting from that opinion. He maintained, however, that I had once been inclined to that hypothesis, or something like it, and appealed to my book. The book was then, and is now, before the public, who may soon be satisfied that it contains no marks of my having ever given the least countenance to an
opinion

opinion so evidently void of all probability. For phlogisticated air wants almost every distinguishing property of fixed air.

It is not imbibed by water, it does not turn the juice of turnsole red, it does not precipitate lime in lime-water (though, during the process, there is a precipitation of fixed air from the common air, which I discovered, and gave an account of in my first volume) and lastly, which makes as manifest a distinction between these two kinds of air as any, they differ very greatly in specific gravity: for fixed air is considerably heavier, and phlogisticated air a little lighter than common air.

The former was the discovery of Mr. Cavendish, and the latter was an observation of my own, mentioned in my first volume, but more exactly ascertained in the second. Dr. H——s, however, not having attended to this as he ought to have done, says, in his *Syllabus*, page 3, that, “phlogisticated air does not greatly exceed pure air in specific gravity.” On the contrary, he will see in my second volume, if he thinks it worth his while to complete his set of so *capital* a work, that, the purer air is, the heavier it is, and the more phlogisticated, the lighter.

Before

Before Dr. H——s lays claim to the discoveries of others, I think he should shew that his *mistakes* are his own. For his notion that fixed air consists of common air and phlogiston is advanced by Dr. Rutherford, in his dissertation on the subject, and I am told was the opinion of Dr. Cullen, from whom Dr. H——s actually had it. In those gentlemen the idea was very pardonable, the subject not having been sufficiently examined; but it has been so fully investigated of late, that so gross a mistake concerning it is now absolutely unpardonable; especially in a person who pretends to be a teacher of philosophy, and who is a supercilious censurer of others.

Our next conversation, which was likewise begun in the presence of Dr. Brocklesby, was on the subject of *acids in the form of air*. I told him that I had pursued what I had before discovered on that subject much farther; having, particularly, made many experiments on the vitriolic acid air, which the readers of my second volume will see were begun at Mr. Trudaine's in France, and compleated presently after my return to England, before I had so much as heard of Dr. H——s; and that I only wanted proper substances from which to expel the other acids in the same simple form, and a proper fluid to confine the nitrous. For the

the *vegetable acid air*, he mentioned several things which he thought would answer, and among others, a *concentrated vinegar*, of his own preparing, which I took, as the cheapest of them; and by the help of it I immediately made the experiments described in the second section of my second volume, acknowledging, as will there be seen, from whom I had the preparation.

After this, I was a little surprized, when, in the last conversation that I had with him, he told me, as a new thing, that he had discovered the *vitriolic acid air*. I replied, "Do you not remember that I told you that I had done the same, the very first time that I was in your company, and that I had materials for a pretty large section on that subject, intended for my second volume?" To this he made not one word of reply.

In our first conversation on the subject of acid air, I asked him whether he could find me any fluid substance that would not be affected by the nitrous acid, which my readers will know to have been a great *desideratum* with me. After some pause, he told me he could, and mentioned *bees wax*. But upon trying it with the strongest nitrous acid that he himself could procure me (and by which he said it

it would be the least affected) it was all dissolved by it, when it was a little heated, and therefore did not answer in the least. I told him of the failure of this experiment; upon which he said he believed that he *did* know what would answer, but he did not tell me. I imagined that he intended to prosecute the experiment himself; and therefore I urged him no farther on that head.

While we were talking on this subject, he shewed me his process for making *spirit of nitre*, which was then going on, to prove that there is much *air* in that acid. But I had not time to consider what I saw, and I can give no good account of it. My own experiments give me a very different view of the subject; and when I attended a course of chemical lectures, delivered at Warrington, by the ingenious Mr. Turner of Liverpool; I was one who assisted in making a quantity of *spirit of nitre*, in a manner not so expeditious, indeed, as that which I suppose is now generally used, but in which I am pretty confident there was no opportunity for any common air to get into the composition of it. I wish, however, to examine this process more particularly, and I think myself happy in having, for this, and other chemical purposes, made more than one

acquaintance, by means of whom I shall soon be able to gratify myself in this respect.

At one time that I called upon Dr. H—s, he had a process going on by which he told me that he procured the *sedative acid*, and I think he likewise said, *in the form of air*. If he can shew any such acid air, it is entirely his own. I have no sort of pretension to it. On the contrary, I am at present inclined to believe that there is no such thing.

As to the experiments which I have made on the *fluor acid*, I question whether I had so much as begun them at the time that my acquaintance with Dr. H—s terminated. These I was enabled to make by means of Mr. Woulfe, without whose generous assistance I could have done nothing on the subject, as my narrative will shew.

At the time of my introduction to Dr. H—s, I had the greatest part of the materials for my second volume, and I told him I should soon make another publication on the subject of air; but that I wanted to complete two courses, viz. on the extraction of air from various simple and compound substances, by a burning mirror in quicksilver, and also by a mixture of spirit of nitre; and I had several preparations

preparations of him for those purposes, as my narrative will witness. Having got an ounce of *mercurius calcinatus per se*, of Mr. Cadet, while I was at Paris, for the purpose of my experiments on dephlogisticated air, which were begun long before that time, I would have had some of Dr. H——s; but found that (tho' he assured me I had every thing of him at prime cost) he could not afford it so cheap as Mr. Cadet. I therefore desired him to make me a quantity of *red lead*, from which substance I had got air about five times as good as common air. When I first mentioned this kind of air to him, he said, "How do you know that it is so pure?" I told him it appeared to be so both by the test of nitrous air, and also by a mouse actually living in it five times longer than in an equal quantity of common air: to which he made no reply.

The first time that I saw him after I had got the red lead, which he had made for me, he said, in the following identical words, "You get no air from red lead." I told him I did, and even air five times as good as common air, such as I had mentioned to him before; but, said he "you get no air from the red lead that I made for you." I told him I did, and air of the same kind, though in a very small quantity. After this it is impossible

able that he should have any pretensions to the discovery of *dephlogistified* air, which is the only discovery for which the evidence of Dr. Brocklesby can be pretended, and even that pretended evidence has intirely failed him.

I first discovered that I could make *dephlogistified* air, and consequently *common air*, from spirit of nitre and earth, when I was at Calne, on the 30th of March 1775; which is a discovery that directly overturns Dr. H——s's doctrine, as laid down in his *Syllabus*, which does not admit of the convertibility of either *earth*, or *acid*, into air. Upon my return to London, after I had sent my letter upon that subject to the Royal Society, I told him that I now knew what common air was, for I could actually make it myself; and at the same time I mentioned the composition. To this he made not one word of reply. Now the air which I shewed to Dr. Brocklesby, and which, he asserts to have been the same with some that he had before seen in Dr. H——s's course, was this very kind of air; having been made with different kinds of earth with spirit of nitre. Now that Dr. H——s should actually have made a species of air, the composition of which, according to the doctrine of his *sylabus*,

labus, just now published, is absolutely impossible, I think my readers must deem to be a little extraordinary. In short, if this discovery concerning the constitution of atmospherical air, be not my own, nothing that I ever did can be so; and if it be not sufficiently proved by these considerations, nothing of this kind is capable of being proved.

With respect to this mistake, however, as well as that concerning the constitution of fixed air, Dr. H——s has nothing to boast; for the opinion that he maintains on this subject is the very same that has always been maintained by almost every body except myself. But so clear are the proofs that I have produced of it, from actual experiments, that I will venture to say, that if Dr. H——s himself does not embrace it very soon, giving up his favourite fundamental doctrine of the *elementary nature of air*, he will be as singular in his opinion, as I have hitherto been in mine. Complete as his knowledge is of all the *seven elements* of nature, comprehending the *omniscible* of natural knowledge, his *ipse dixit*, delivered in his oracular syllabus, is not of so much authority, except perhaps with himself, as that of fact and experiment.

So much is Dr. H——s's doctrine on the subject of air the reverse of mine, which makes *freedom from phlogiston*, exactly to correspond to *purity of air*, that, in a conversation with me, he maintained that air might have *too little*, as well as *too much* phlogiston. He did not think proper to explain himself on the subject; and I can only assure him that I know no such air. Let him produce it if he can, *et erit mihi magnus Apollo*.

In the same conversation in which I told Dr. H——s that I had discovered the real constitution of atmospherical air, I told him that I thought I had also discovered the composition of *fixed air*. Upon this he smiled, with a kind of triumph, saying, "You are convinced then, at last, that fixed air is a compound." I told him I was, because I thought I had discovered in what it consisted, viz. some modification of spirit of nitre, and phlogiston, and perhaps some other principle. Upon this subject I am still in suspense, waiting for more experiments. But allowing that I had changed my opinion, which I have never been averse to acknowledge, I have not yet adopted *his opinion*, viz. that fixed air consists of common air and phlogiston; so that I am no convert of *his*, but to *myself*, the opinion being, as far as I know, peculiar to myself; and therefore Dr.

H——s

H——s can have no foundation for giving out, as I am informed he has done, that I have changed my opinion on the subject of fixed air, in consequence of the conversation I had with him.

So far was Dr. H——s from being communicative to me of his knowledge, that he was not always in the humour, notwithstanding his liberal promises, to let me have the materials with which he could have furnished me for my own experiments, except on such terms as he saw I could not comply with. I once wanted a small quantity of such *phosphorus* as Mr. Canton made; and as I saw that he had just made a quantity, of the excellence of which he boasted very much (as, indeed, he did of almost all his preparations) I begged that he would let me have a little of it. He said I should, if I would promise to give no part of it to any body else. I told him that I had no intention of communicating it to any body, but that I did not like to lay myself under the obligation of such a promise; and therefore I had none. Going to work myself, and following Mr. Canton's directions, I found no difficulty in making it sufficiently well for my purpose.

SECTION IV.

Observations on Dr. H——s's Syllabus, as far as it relates to the doctrine of air.

In order to throw as much light as I possibly can on the subject which I have undertaken to discuss, viz. whether it be probable that I have borrowed any of my experiments of Dr. H——s, it may be useful to consider whether his doctrine concerning *air*, contained in his *Syllabus*, lately published, be such as may be supposed either to have *suggested*, or to have *resulted from* those experiments. If our *conclusions* be totally repugnant, it will hardly be thought probable that our *premises* were the same. Now that our conclusions are totally repugnant, will be evident to any person who shall inspect his *Syllabus* and my *second volume*; and it is something remarkable that our opinions are, in no respect, so much the reverse of each other, as in what relates to that very species of air, the discovery of which, the evidence of Dr. Brocklesby (if it could have determined any thing at all) would have given to Dr. H——s.

It

It was exceedingly fortunate for me, that Dr. H——s happened to publish this Syllabus of his, at this very seasonable time; as, without it, my defence could not have been so complete as I am now able to make it; so that, without having ever thought of the matter, I find myself possessed of the earnest wish of Job, *My adversary has written a Book*. For now, out of his own mouth I can convict him; and so long as there remains a single copy of that precious *Syllabus*, I must stand acquitted, and he condemned.

In this section I propose not only to point out the essential difference between Dr. H——s's opinions and mine, but, that my reader may derive some little advantage from the dispute, I shall, as I have done in the preceding sections, at the same time, shew how exceedingly frivolous are his objections to my doctrine, and how very crude, futile, and contrary to fact are his own; not forbearing to *laugh where we must*; since there is, in truth, very little room for *candour*.

In this curious syllabus, Dr. H——s repeatedly calls *acid air*, *alkaline air*, and *nitrous air*, p. 21, 27, *conceits*; alluding, no doubt, to myself, who first adopted these terms. Now

this must have arisen from his ignorance of the *nature and use of words*, as if any person was not at liberty, (like Capt. Cook, or any other navigator) to call a thing which had no name before, by whatever name he pleased; or as if the *nature of the thing* was affected by the choice of a *term*. If instead of *air*, I had used the word *emanation*, *vapour*, *principle*, or Dr. H——s's more favorite term *element*, would there have been any real difference in the substances, so differently called? or, by calling them *air*, are the *facts* that I have discovered relating to them the less true.

Besides, Dr. H——s himself uses the term *inflammable air*, without the least scruple, though, according to his theory, there is not a particle of air in that fluid. For he says, after me, p. 43, "that it consists entirely of acid and phlogiston." This was my own conclusion from the experiments mentioned in my first volume; but I have now rejected that opinion, because I have since that time procured inflammable air from metals by *heat only*, without employing any *acid* whatever. Dr. H——s, however, is very welcome to keep my old opinion, if he prefers it to my new one. But whichever of the opinions he adopts, he is certainly obliged to me for it.

Now

Notwithstanding Dr. H——s thinks proper to call *nitrous air*, *acid air*, and *alkaline air*, mere conceits, and to consider almost all my originality as a mere *knack to make plain things mysterious and obscure*, I cannot help thinking that if the conceits had been *his own*, and if he himself had had as good a *knack* at these things as I have, he would have thought the conceits to be very pretty ones, and would have been not a little proud of his knack of striking them out. And it is possible, that if he had produced any such conceits of his own, he would not have looked with such envious eyes on those of others. On this account I really wish that he may have better fortune in his inquiries; for then, while he is exulting in his own discoveries, and *making mountains of mole-hills*, other quiet people may hope to enjoy their own property unmolested by him; unless he should resemble the lion in the fable, who, though he had no hand in catching the stag, challenged all the four quarters of it for himself.

I have observed that Dr. H——s has done me the honour to adopt several things from my first volume, but I see nothing common to us both of what is contained in my *second* volume, except the mention of *vitriolic*, and

acetous air, which terms he heard me make use of, and which he calls *conceits*, and an intimation that he can explain the phenomenon of *detonation* without supposing a destruction of the acid. This was an easy and necessary result from some of my new experiments, especially those that relate to dephlogisticated air, in the discovery of which he certainly had nothing to do.

His whole philosophical theory rests upon the foundation of there being distinct *primary elements* of matter, of which he makes seven, viz. *earth, water, alkali, acid, air, phlogiston, and light*. All these, he asserts, p. 9, to be *impenetrable, immutable, and inconvertible*. But nothing can be more uncertain, or hazardous, than such a position as this. We are far from being sufficiently advanced in the knowledge of nature to pronounce concerning its primary constituent parts.

Dr. H——s more especially asserts, p. 17, that *the pretended conversion of water into earth is an erroneous notion*. But while he pretends to have considered the experiments of Boyle, Borrichius, Wallerius, Leidenfrost, Margraaff, Eller, and Lavoisier (which is calculated to convey an idea of his extensive reading) he has overlooked the more decisive

ex-

experiments of his countryman; the ingenious Mr. Godfrey, who converted the whole mass of a considerable quantity of distilled water into a perfectly dry earth. For my own part, I see no reason to doubt of the fact; and what is much more, Mr. Woulfe, who is unquestionably one of the ablest and most judicious chemists of the age, says that he has seen enough, in his own experiments, to make him perfectly satisfied with respect to it.

Dr. H—s calls *earth* an inconvertible element, but I will undertake to convert the whole of a quantity of earth into what he shall be obliged to call *air*; and, provided it be pure earth, by which I mean free from phlogiston, it shall be respirable air.

He says, p. 44, that “the nitrous acid prevents the formation of inflammable air, in all circumstances yet discovered;” whereas, if he had read my first volume with care, he would have found that, by a very easy process, I can always make inflammable air from the nitrous acid, viz by putting iron, or liver of sulphur into nitrous air.

To mention a few other articles in this curious syllabus that do not relate to air, but some other of his elements: he says, p. 46,

50, 51, that "fire consists of light and phlogiston, and is not a certain motion of matter; that *blaze*" (I suppose he means *flame*) "is a mixture of fire and phlogistic matter, which has not yet formed fire, p. 54; that light is not" what Newton supposes) "a matter sent forth by the sun, or stars, or planets, p. 65; that darkness is not the absence of light, or any privation of light, ib.; that illumination, commonly called light, and darkness, are with respect to light, what sound and stillness are with respect to air, p. 66; that our sense of colour is our perception of the modifications of the vibrating motions of light, and that the seven prismatic colours are, with respect to light, what the seven tones are with respect to air, p. 72; that specific gravity and density are not commutable terms; that there is not necessarily more matter in a cubic inch of glass, than in a cubic inch of rosin, for that gravity depends as much upon the species of the gravitating matters as upon the quantity of them, p. 48; that phlogiston does not gravitate, and that it has a power whereby it counteracts the gravitation of other matters, p. 47."

These

These and suchlike long-exploded, and crude notions (so many of which I believe were never thrown together into the same compass since the age of Aristotle or Cartesius) are delivered in a manner and phrase so quaint, and a tone so solemn and authoritative, as gives me an idea that I cannot express otherwise than by the term *Philosophical Empiricism*.

SECTION V.

MISCELLANEOUS OBSERVATIONS.

I shall begin this section with some general observations on the *nature of the accusation* brought against me, and of the *evidence* by which it is supported.

My acquaintance with Dr. H——s commenced on the 6th of February 1775; and he says (for I happen not to have any note of that memorable æra myself) that it had been discontinued nine months, on the 3d of December following. It must, therefore, have terminated in the beginning of March. But I believe he is mistaken about two months, and that it was in the beginning of May; so that I give him two months more than he claims. Three months, then (a great part of which I spent in the country) my acquaintance with Dr. H——s lasted.

The second edition of my *Treatise on air* had been published some time before I had so much as heard the name of this gentleman, to whom it has been said, and with very great confidence, that I owe *all my discoveries*; so that he
can

can have no claim to any thing mentioned in that volume. At the same time, also, it is well known to my friends, and I mentioned it to Dr. H——s himself, the first time I saw him, that I had materials for a *second publication* on the subject. I must, therefore, at that time, have had the materials for the *bulk* of the second volume, I suppose about three fourths of it. The remaining fourth part, therefore, is all that can lie open to his claims; and even with respect to this, he will find that I am able to produce evidence, that every course of experiments, of any consequence, was begun, and pretty far advanced, before I knew him; so that I had little to do besides merely *completing* them, excepting what relates to the *vegetable acid air*, which is a thing of very little value, and the experiments on the *fluor acid*, which Dr. Brocklesby, the only evidence that has yet appeared against me, does not pretend to have seen with Dr. H——s.

In fact, therefore, there remains nothing of any value for him to lay his hands upon, except the completion of the discovery concerning *dephlogisticated air*, which I had begun before I knew him; and though his friend has asserted, in general, that he saw *all* the experiments I shewed him (and these were among them) with Dr. H——s; the circumstances of that

that fact have been stated to be such, that I am satisfied my reader must be something more than prejudiced, to imagine that it was even possible he should have seen them.

When I first mentioned the facts to Dr. H——s, he even positively denied that any air could be got from the substance from which I actually procured that specific kind of air, and the *necessary conclusions* from these experiments are not only not found in his printed *Syllabus*, but are the very reverse of the fundamental doctrines of that syllabus.

Now I will venture to say that whenever any other article is examined, his claim to it will appear to be equally unreasonable and absurd. The book, however, will soon be before the public, and he may then cast his rapacious eye over every paragraph of it, and let him distinguish his property there, if he can.

I am very confident, that if the dates annexed to any of the articles were concealed, and he was required to name his own, he would just as soon take what was done before I knew him, as what was done after that time. In fact, he has an equal right to *all*, or *none*.

It

It seems, however, very extraordinary to me, that he should, at the same time, despise all that I have done, calling my discoveries mere conceits, and say that I am possessed of nothing but a *knack of rendering what was intelligible before, mysterious and obscure*, and yet covet those things for himself. The second volume, I can assure him, contains nothing but *more conceits*, of the same kind with those in the first, and nothing is exhibited in it but the exercise of *the same knack*, whether of darkening or enlightening things, that was displayed in the former volume.

According to Dr. H——s's account of the use that I have made of the discoveries of chemists, neither himself, nor any other person, has been really injured by me; for I have only disgraced myself. What reason, then, can he have to complain? Let him only publish his experiments, which are so very intelligible; and if it appear, by comparison, that mine are only calculated to throw darkness upon his light, their credit cannot last long; and every thing that I have done, contained in both my volumes, must vanish before his publication, like Satan, the prince of darkness, at the touch of Ithuriel's spear. If all that I have done be what he represents it, a mere *imposition upon the public*, why cannot
he

he be content that I should have all the infamy of it to myself. Is it, that he is willing, out of a principle of compassion, to share the burden with me?

As he says, that *I have treated others as I have treated him*, I think I may safely conclude, that I have only treated him, as I have treated others; and therefore that I have stolen no more from him, than I have done from others. Now, as my works are open to the public, let him shew what it is that I have taken from *others*, without acknowledgement. But as I am confident that all the world will acquit me of any thing like plagiarism with respect to them, they will as readily acquit me of the same charge with respect to him.

During my acquaintance with Dr. H—s, he was perpetually talking of his *discoveries in general*, but without distinctly specifying them; so that I do not retain a single idea of any that he has ever made, and I have never heard the least mention of any of them except from himself*. Indeed the great burden of his discourse with me was, that people came perpetually teizing him with questions, took up

* I must except a single circumstance, mentioned in a late volume of the *Philosophical Transactions*.

his time, got hints of discoveries from him, and then published them without making any acknowledgment. But I remember that he never mentioned the name of any of those persons. I now publicly call upon him to name them, that we may know one another, and compare notes; for I fancy we shall all find ourselves in the same situation, that there has been much cry and little wool; that these many persons, all publishers of experiments, have written from their own funds, and that we should have had a very scanty supply, if we had only had Dr. H——'s hints, and voluntary communications, to depend upon.

Chemistry, however, being a wide field, and myself having had access only to a one fruitful corner of it, I, in the great simplicity of my heart, entertained no doubt, but that while I was exploring one place, he was doing the same, and with the same success in another; and there was certainly room enough for us all. But I now begin to suspect that (whether through his too great eagerness to catch at every thing, and secure the whole field to himself, or through some other cause) not having had the good fortune, in fact, to lay hold of any thing himself, he has been seized with a longing desire to snatch a few of

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the flowers that I and others have been very quietly gathering; thinking, that, out of so great a number, he might, without fear of detection, secure a few: and could he have contented himself with enjoying his pretensions with more privacy and discretion, he might have succeeded better.

Now could we *all*, on whom he has been making his depredations, only know one of another, though we should only be half a dozen of us (and yet, from his own account, which pretty much resembled that of Falstaff, I should think that we cannot be less than a *score*) we might perhaps, by consulting together, hit upon some method of satisfying this unfortunate experimenter. We might each of us agree to make him a voluntary contribution out of our common stock. For my own part, I love my rest and peace so much, that rather than have such another affair as this, I would freely surrender to him one or two leaves of my *Register*, and a few good *hints* to work upon into the bargain.

Dr. H—s seems to be much offended at the *rapidity*, as he calls it, of my philosophical publications. Now every man has a peculiar *manner*, and a peculiar *fate*. No two men are,

in

in all respects, alike. He is not what I am, nor am I what he is. It may be my fate to be a kind of *comet* or flaming *meteor* in science, in the regions of which (like enough to a meteor) I made my appearance very lately, and very unexpectedly; and therefore, like a meteor, it may be my destiny to move very swiftly, burn away with great heat and violence, and become as suddenly extinct. Let Dr. H——s, therefore, if he be wise, keep out of my way; let him wait till my fated period arrive (which, in the nature of things, cannot be far distant) and he may then, after seeing my fall, like a slow sober-moving *planet*, attended by his faithful satellite Dr. Brocklesby, perform his own revolution unmolested, when I shall be involved in darkness.

As a circumstance that will have some weight with our judges, who are to decide whether it be more probable that the discoveries in question be mine, or Dr. H——s's, I think he should be required to produce before them any discoveries concerning air, that are *unquestionably his own*, as a specimen of his abilities in this way; or, at least, discoveries of some kind or other. Thus, when the wasps claimed the combs and the honey of the bees, they were required by their equitable judge, in *Æsop*, to produce such combs themselves.

I shall now conclude this appeal to the public with a letter to my *accuser*, and another to his *witness*; after which I shall submit the decision of my cause to a jury of our peers, the *public*; acknowledging, whether Lord Mansfield will agree with me in this, or not, that they are competent judges both of the *law*, and of the *fact*.

To Dr. H——s.

Sir,

It is something odd that the subject of the only paragraph in my letter which you thought *required an answer*, is the only one which it is impossible for me to make a reply to in yours. For, indeed, as you say, *our notions of honour differ so very widely*, that it would be going too far back, for a correspondence by letter, to come at any *common principles* on which we might argue. Besides, the public may have notions of honour different from us both, and they will judge between us. To which of our maxims they will most incline, time will discover.

You say that, “if any other gentleman had
“proposed to you the question that I did, an
“answer

“ answer would have been necessary.” Now, as I cannot pretend to be any other person *now* than I was *then*, I imagine you will still think an answer to *me*, unnecessary; but as with respect to the *public*, or to *yourself*, you may, possibly, think it *expedient*, and your time may be too much taken up in the prosecution of your immensely valuable discoveries, even to read *the whole* of this pamphlet, I shall, in a few distinct paragraphs, recite all that it particularly concerns you to reply to.

1. You must distinctly recite those discoveries of yours, which you charge me as having published as my own; proving that you had published them before me, and that *I knew* of your having made them at the time of my publication. You say that “ If you should answer my question, you would commence it “ with comparisons of the dates of my rapid “ publications, with the dates of your courses “ of chemistry.” Now this is more than necessary, unless you can prove that I knew any thing of you, or of your courses, before the 6th of February last, and can find in the *two first lectures of the course*, which you began on that day, the seeds of my discoveries subsequent to the date.

But I find, by your friend Dr. Brocklesby, that your very *first course* began in June 1774; whereas my first volume on air was published some months before that date; so that, tho' your voice could have been heard from your laboratory in Greek-street, Soho, London, to my fire-side at Calne, in Wiltshire, I could not have profited by your instructions. My unfortunate *conceits* were then all abroad, and, to my everlasting shame, were at that time well known to philosophers in many different parts of Europe, and long before that time, the Council of the Royal Society, wanting the wisdom of your advice, had been so infatuated, as to have conferred upon me their annual prize-medal for about one half of those that are contained in that first volume. Misled by their ill-founded applause, I have gone blindly on in the same walk, till my conceits are now more than four times as many as they were at the time of my first publication.

2. After you have made good your *first* charge of plagiarism, with respect to *yourself*, please to prove your second also, viz. my plagiarism with respect to *others*.

3. The

3. That this altercation of ours may be of some use to the public, and to make it worth their while to give us a hearing, I wish you would discuss the several topics on which your philosophical notions and mine differ. It is to be wished, more especially, that you would prove your favourite doctrine, that fixed air consists of common air and phlogiston; that acid air, alkaline air, and nitrous air, &c. &c. &c. are mere *conceits*; and that your fundamental principle of the absolute inconvertibility of what you call *elements* into each other, is well founded, especially that *earth* is not convertible into *air*, as I assert, and you deny.

4. It would be particularly edifying to the public, if you would favour them with an elucidation of your extraordinary *Syllabus*, a few things in which I took the liberty to point out, as *wanting some illustration*; as your notions concerning *fire, light, phlogiston, &c.* But perhaps you may, in your great prudence and discretion, think it quite sufficient, if, for the present, you can give satisfaction to your *pupils* with respect to them: and I own, upon reflection, it would be unreasonable to require of any person of your description, that he should publish to the world all the *secrets* of his *Art*.

5. As to the particulars which only passed in conversation between our two selves, and which have not yet been communicated to the public, not even in your amazingly-comprehensive syllabus, as concerning the *sedative acid*, *air that has too little phlogiston*, &c. &c. &c. it no way concerns the public; but perhaps you may chuse, while your hand is in, to clear up those matters as well as the rest.

6. Above all things I must insist upon it, that you specify the *names* of the *many persons* who have behaved towards you with the same baseness and ingratitude that I have done; that the public may judge of the credibility of your charge against me, by comparing it with your charge against others, probably much more respectable persons than myself.

7. As to your personal *behaviour* to me, and your *reasons* for it, you may give just what account you please. As my character is pretty well known, those who are acquainted with me will judge whether your account be probable or not, and though I do not pretend but that my memory may fail me with respect to some *circumstances* of things, I think a man will hazard too much who shall

shall charge me with any wilful misrepresentation of a fact. I would not for the sake of all the reputation that a man can get by philosophy, or by writing, have the feelings of that man who shall charge me with having told a *deliberate falsehood*; for if he have any knowledge of me, he must, at the same time, be conscious of telling one himself; asserting what he does not believe.

I am, Sir,

Your humble servant,

Shelburne-house,

8 Dec. 1775.

J. PRIESTLEY.

P.S. As I have now the honour of introducing you to the Public, as Dr. Brocklesby introduced me to you, I hope you will not immediately adopt the style of your letter to me, with which our intercourse terminated, but rather that which you first used to me, as expressive of that deference and respect which you thought due to a new acquaintance.

If in any part of this pamphlet, or letter, I have *inadvertently* offended you, I shall hope to be favoured with a *remonstrance* on the subject. The *title*, at least, cannot displease you. In this I have not been partial to myself; for
whenever

whenever the publication is mentioned, it will be called *my Philosophical Empiricism*, and not *yours*. Your friends, therefore, may be expected to circulate it as well as mine.

TO DR. BROCKLESBY.

Dear Sir,

I am sorry to observe that, in your last letter, you drop the usual stile of friendship, in your address to me; but this shall not make me discontinue it with respect to you. For though, by means of your indiscretion, I have long lain under a great load of odium, and you have occasioned me a great deal of trouble in consequence of it, I believe it was very far from being your intention to injure me: and whatever I may think of you as a *philosopher*, or as a *writer*, I shall always respect you as a *gentleman*. Besides, your known attachment to the cause of Liberty, would alone, if you had nothing else to recommend you to me, disarm, in a great measure, my resentment.

I have no objection even to your entertaining whatever regard you please for your friend Dr. H——s, who, how deep soever he may be in *philosophy*, and how happy a talent soever he may possess of communicating

municating his own clear ideas to others, (of which yourself, who have so long been his pupil, have exhibited the most satisfactory proof) certainly has not, in return, received from you all the accomplishments of a gentleman; being manifestly deficient in the first rudiments of that character, viz. good manners, as his behaviour, and his letter to me, will testify.

I would observe, however, that several parts of your letter might very justly give me offence, especially your intimating that by proposing my questions I meant not to clear up the subject (though nothing could be better calculated to answer the purpose, as the issue has proved) but either to puzzle you, or lead to farther perplexities. For they could not have puzzled or perplexed any man who was a competent evidence in the case.

All the particulars of your letter that are true (for several of the articles are notoriously false) are either things that have been long known to all philosophers, or are contained in my first volume; and yet, after reciting them, you bid me "ask myself whether any
" thing that I shewed you at Shelburne-house
" could appear novel to you; since my giving
" new

"new names to what you had seen before,
"could not constitute any new discovery."
Now I shewed you several very remarkable experiments, of which nothing is so much as hinted at in your letter; and therefore, on your own testimony, they must have been quite new to you, whether you were aware of it or not.

You complain that you have been drawn by me into a dispute against your will; but, Dear Sir, is it not rather you who have drawn *me* into this dispute? And I do assure you it is much against my will. A very serious accusation has been brought against me, respecting, not, as you represent it, *the exclusive right to a philosophical discovery*, but affecting my *character as an honest man*, and you are the only person who have stood forth in support of this accusation. Can you then seriously blame me for calling you to prove what you acknowledge you have asserted, and for proposing such questions as were evidently necessary to ascertain the validity of your testimony? A moment's reflection will convince you that, in justice to myself, I could not have done otherwise.

You

You must now, Sir, give me leave, in return for your anecdote concerning Sir Isaac Newton (which you have intirely misapplied in my case) to tell you a story which you cannot misapply, and I hope it will not be lost upon you on a future occasion.

A Chinese Mandarin had procured an European reflecting telescope, and a friend of his, wishing to have another exactly like it, put it into the hands of a Chinese workman, who was famous (as many of the Chinese are known to be) for the imitation of any thing he saw. Accordingly, having got the instrument into his hands, he surveyed it with great attention, took it to pieces, and carefully measured the dimensions of every part. He then made a tube of the very same size, and mounted and polished it, so as not to be distinguished from the other; and with respect to the inside of it, he put pieces of polished metal, and pieces of transparent glass in their proper places, and precisely at their proper distances from each other; but without attending to any more exact curvatures of their superficies than his eye, which was a very good one, could distinguish: and then concluded that he had completely constructed the telescope. And certainly a Chinese Dr. Brocklesby

lesby would have said that *they appeared to be nearly the same* ; and yet the European instrument would magnify remote objects with great distinctness, whereas, through the Chinese telescope, nothing could be seen at all.

Now this I take to have been the difference between Dr. H——s's experiments and mine, and I hope that the next time that you shall see a man standing by a tub of water, or a basin of quicksilver, with jars and phials, &c. before him, filled partly with air and partly with water, with a lighted candle, and a variety of little implements at hand, and transferring his different kinds of airs, with some degree of dexterity, from one vessel to another, a *red* colour appearing here, and a *white* one there, you will not be so ready to affirm that the operator was *instituting* the very same experiments that you saw at Shelburne house.

As a story frequently begets its own likeness, and examples of this kind may be useful to you in the way of *apology*, as well as of *admonition*, I shall, while I am in the vein for it, tell you another.

Your expressing no sort of *surprize* at seeing my new experiments, reminds me of the
in-

indifference with which Tobiah, a very sensible native of Otaheite (so that it is no disparagement to you to be compared to him) saw a *horse* for the first time at Batavia, when it was imagined that he would have been struck with the greatest admiration, especially as he was remarkable for his curiosity, and his attention to every thing that appeared new to him. But when he was asked by Mr. Banks, who told me the story, if he did not admire that noble animal? he said, "No, for there was nothing extraordinary in it, except its size, as such animals were common enough in his own country." Upon inquiry it appeared that he took the horse to be nothing more than a large dog.

Unfortunately, this Otaheitan Dr. Brocklesby did not live to return to his own country. But supposing him to have returned, and his countrymen gathered about him, asking him whether he had seen any thing *new* in his travels; he would have said, "No, these people (who are said to be *philosophers high in modern rank*) with great trouble and expence, run over the world, on purpose to make what they call *discoveries*, and I suppose there are people on whom they can impose. But as far as I see, and I have been
" long

“ long enough with them to judge, they only
“ amuse themselves, and the world, with giv-
“ ing new names to things that we all know as
“ well as they do. They pretended to shew
“ me a most extraordinary animal, and thought
“ to have surprized me exceedingly with the
“ sight of it; but though they called it by a
“ name that I had never heard before, and
“ that I cannot now recollect, you may depend
“ upon it, it was nothing more than a dog,
“ only a little larger than our dogs generally
“ are. It had only four legs like ours, one
“ head, one tail, and a couple of ears, and it
“ seemed to run at the same speed. As for the
“ species of the animal, let them pretend what
“ they will, be assured by me it was the very
“ same.”

I hope, dear Sir, you will not think the worse of me for endeavouring to give a turn of pleasantry to an affair that, some time ago, wore a pretty serious aspect. Believe me, I retain no animosity against you. I have even no objection to seeing or conversing with you as usual. Only I fancy it will be equally agreeable to us both to say nothing, for the future, about philosophy, or Dr. H——s, but rather

to talk about America, and our common friend
Dr. Franklin.

I am, Dear Sir,

Your very humble Servant,
Shelburne-house,

Dec. 9, 1775.

J. PRIESTLEY.

P. S. Had you listened to the gentle and
timely admonition of Dr. Hunter, at the time
of your making the declaration above-men-
tioned, viz. "I suppose that what Dr. H—s
" shewed might be *something like* these experi-
" ments, but they might notwithstanding, in
" reality, be very different from them," it
would have been happy for me, and perhaps
also for yourself, and even for your friend Dr.
H—s, in whose reputation you so warmly
interest yourself.

THE CONCLUSION.

I have now made the best defence that I can to the *general* and *indistinct charge* that has been brought against me, and am waiting (with how much anxiety may well be imagined) for the *particulars* of my accusation, of the nature of which I am just as ignorant as my reader himself. I take it for granted, however, that it relates to some of the articles contained in Dr. H——s's syllabus, which was intended, no doubt, to comprise the result of all his discoveries, those that I have been pilfering, as well as the rest; and indeed it takes in the whole compass of philosophical knowledge. But then, among such an immense number of discoveries, great and small, how can I determine which of them it is on which he will found his charge.

I believe I must, in this case, have recourse to the method formerly used in taking the *ortes Virgilianæ*; and, as it is possible, though not through a *deficiency*, yet through a *redundancy* of his articles of accusation (which is no less

less embarrassing) Dr. H——s himself may be as much at a loss as I am, I would recommend the same method to him; and if he should not happen to know what it is (as the process is not a strictly chemical one) I will tell him, that he has nothing to do but to open the book at random, and the first paragraph that he shall casually cast his eye upon, is the article wanted.

I am not lawyer enough to know whether it would avail me at all in this case, to turn *informer* against my prosecutor, or I could prove that not a single article mentioned in Dr. Brocklesby's elaborate letter (which I doubt not contains a full and accurate account of all the *recondite* doctrines, and profound discoveries, delivered in the Greek-street lecture) whether *true* or *false* (for the account consists of a due mixture of both) belongs to Dr. H——s.

If this will not avail me, and my defence, after all, be deemed unsatisfactory, I shall be anxious to know to what *punishment* I shall be sentenced. For if my crime should be adjudged to be any thing more than *petty-larceny*, I am apprehensive that, as we have no colonies for the convenience of transporting felons now, I shall inevitably be *destined to the cord*.

I do not know whether my *nativity* was ever regularly *cast*; but if it was, I am confident it must have appeared, that I was born under the malignant influence of some or other of the *planets*, to which the old chemists paid a more particular devotion; and it is well known that they had much recourse to the planets. For I cannot otherwise account for my being so exceedingly obnoxious to *lecturers in chemistry* as I have been. If I might adopt the doctrines of my Scotch antagonists, I should say they seem to be possessed of an *instinctive antipathy* towards me, and to fall upon me as naturally as the wild asses, in Arabia, fall upon the horse, or, if they like it better, as the wild horses of Arabia fall upon the ass.

For, a few years ago, I happened to be but a quarter of an hour in company with another celebrated lecturer in this branch of liberal science, in this metropolis, and I narrowly escaped being brought into a scrape as bad as this that I am now in with Dr. H——s. It was indeed much of the same nature, and, as far as I can foresee, would have ended as this is likely to do. Nay it looked much more formidable at its outset. For I was informed not only that I had published discoveries communicated to me in that unlucky quarter of an hour, without

out any acknowledgment, but a publication was threatened of all my *plagiarisms*, which, as I was then but young in this business (and not case-hardened, as I now am by all kinds of abuse) would certainly have overwhelmed me. And the learned lecturer (though I believe he never acted in concert with Dr. H——s) expressed even greater contempt of my experiments than Dr. H——s has done, and in a style equally correct and elegant.

When, however, the fact was inquired into, it only appeared, that I had not given to an excellent philosopher, with whom I am now better acquainted, an experiment, which, as the chemist described it, was *not fact*, and which, as it ought to have been described, I had not claimed to myself, but had given to another person, who had actually made the experiment, and had published an account of it long before.

Having related the particulars of my own conduct, and my own experiments, as far as the purpose of my defence requires,

—*Sua narret Ulysses.*

OVID.

F I N I S.

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